'White Man's Burden'? A Field Experiment on Generosity and Foreigner Presence*

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Abstract

Can the presence of white foreigners influence measured behavior in developing countries? We experimentally vary foreigner presence across behavioral games conducted in 60 communities in Sierra Leone, and assess its effect on standard measures of generosity. We find that foreigner presence substantially increases player contributions in dictator games, by an average of 19 percent. Using household and village survey data, we show that the treatment effect is smaller for players who hold positions of authority, which suggests that perceived power differentials between players and the experimenter, based on identity, plays a role in mediating this effect. We also find that subjects from villages with greater exposure to development aid give substantially less, and are more inclined to believe that the behavioral games were conducted to test them for future aid. These findings suggest that behavioral responses to researcher identity are in part related to expectations regarding development assistance. More generally, our findings hold implications for measuring generosity and the design and administration of behavioral experiments in developing countries.

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1 Introduction

Over this past decade, there has been a dramatic rise in the use of field experiments and games to measure human behavior across various nations. Yet, little is known about the extent to which the identity of the researcher influences subject behavior, potentially biasing behavioral measures. The role of researcher identity is particularly important in a developing country context, where the researcher may differ dramatically from the subject in terms of race, ethnicity and nationality.

While previous analyses have documented experimenter effects associated with the presence of a researcher, this paper examines whether the race and nationality of a researcher can influence measured behavior, in the context of a developing nation. Specifically, we conduct a field experiment which randomizes the presence of a white foreigner in the administration of behavioral games across 60 villages in rural Sierra Leone and assess its impact on generosity as measured in dictator games.[1] We find that the mere inclusion of a white foreigner on the research team increases the total amount given in dictator games by 19 percent.

The subjects in our study are randomly sampled villagers who played three different types of dictator games: one in which they gave anonymously to members of their own village; a second in which they gave non-anonymously within their own village; and a third in which they gave anonymously to members of another village. We find that the 'white-man' treatment effect is largest in the case of anonymous own-village giving (where the increase is 23 percent), and diminishes significantly with non-anonymity, when player allocations are made public.

Previous studies have documented that subjects alter their behavior in experiments because they are being observed, an idea coined the 'Hawthorne effect' (Adair et al., 1989; Diaper, 1990; Levitt and List, 2011; McCarney et al., 2007). One aspect of this phenomena arises from "experimenter demand effects," in which subjects alter their behavior to conform to the perceived desire of the researcher (Masling, 1966; Levitt and List, 2007; Nichols and Maner, 2008). Along these lines, Hoffman et al. (1994) find that fewer players act generously in a double-blind dictator game, where researchers do not observe the amount given. Haley and Fessler (2005) also find that the proportion of non-zero givers in a dictator game increases when a pair of eyes are displayed on the computer screen where subjects make their allocation.

While these studies show how the mere presence of researchers can alter subject behavior, ours is the first to experimentally identify the role of the researcher's identity in a behavioral game – and one that is carried out in the field. By showing that measured generosity varies not just due to the presence of researchers, but based on who the researcher is, we document that experimenter demand effects may vary based on the race and nationality of the researcher.[2]

[1] We focus on dictator games, since this is the most commonly employed measure of altruism in behavioral experiments.

[2] Other studies have found that researcher characteristics influence subject response in survey data (Miyazaki

Our results are also related to analyses which show that between subject anonymity affects measured altruism (Andreoni and Bernheim, 2006; Charness and Gneezy 2008; List et al., 2004; Masclet et al., 2001; and Rege and Telle 2004). Consistent with these studies, we find that giving is higher when the game is non-anonymous, and allocations are known to community members. However, we additionally examine the interaction of non-anonymity and the white foreigner treatment, and posit that non-anonymity diminishes the 'white-man' effect as reputational concerns regarding other community members erode reputational concerns regarding giving in the presence of the white foreigner.

The findings in our paper also link to the literature examining how player characteristics such as ethnicity and nationality determine generosity. For example, Adida et al. (2011) conduct dictator games in France, and show that the generosity of the rooted French toward Muslim immigrants declines with an increase in the number of Muslim immigrant players. Burns (2004) find that in experimental settings all racial groups in South Africa demonstrate less trusting behavior towards black partners. Habyarimana et al. (2007) find an interaction effect between player's ethnicity and subject-anonymity in Uganda: players give more to co-ethnics only when the dictator games are non-anonymous. In contrast, we focus on the ethnic identity of the researcher. Studies have also found that players display more generosity toward those who are socially closer to them (Bohnet and Frey, 1999; Branas-Garza et al, 2005; and Charness and Gneezy, 2008; Leider et al., 2009). Consistent with this literature, we show that overall giving is lower when the proceeds go to another village, rather than the players' own village.

While documenting the effect of the 'white-man' treatment on measured behavior is the main finding of this paper, we are also able to link our behavioral games outcomes to a rich set of village and household level survey data, allowing us to explore potential mechanisms through which this treatment affects measured generosity. We uncover substantial heterogeneity in the magnitude of the treatment effect based on the subject's power and authority within their village, as measured by whether they are a chief, or from a household that leads powerful organizations called secret societies. This suggests that perceived power differentials between subjects and the experimenter, based on identity, play a role in mediating the effect on measured generosity.

We also find that the treatment effect is substantially smaller among villages with more prior exposure to development aid, as measured by longer engagement with NGOs. In addition, subjects from these communities are also more likely to believe that the purpose of the games was to test the community for aid disbursement, when exposed to the treatment. This pattern of results suggests that those with greater aid experience give relatively less in response to the

and Taylor, 2007; Rosenthal, 1976) based on a wide range of characteristics including race (Cotter et al., 1982; Finkel et al., 1991; Hyman, 1954; Mensch and Kandel, 1988; Reese et al., 1986; Anderson et al., 1988; Webster 1996), education, income (Bailar et al., 1977), and religion (Blaydes and Gillum, 2012). In addition, Zwane et al. (2011) show that the mere act of being surveyed itself can subsequently change household behavior.

white-man treatment, to signal the need for future aid.

The remainder of the paper is organized as follows. Section two provides information on the relevant institutional context of Sierra Leone. Section three discusses the experimental design and empirical strategy. Section four contains a description of the survey data and provides descriptive statistics. Section five presents the results, and section six concludes.

2 Institutional context

Two features of Sierra Leone are relevant for this study: its extensive exposure to foreign aid; and its informal authority system, including the strong local power base of chiefs and secret societies.

Sierra Leone suffered a long and brutal civil war, spanning from 1991 to 2002. The war contributed to the escalation of poverty, leaving the nation amongst the ranks of the world's poorest and least developed.[3] With the end of the conflict, the country experienced a surge in foreign aid (Fanthorpe 2003). Official Development Assistance as a proportion of GNI peaked at 43% in 2001 but remained as high as 23% in 2009, making it the 9th most aid dependent nation in the world (OECD, 2012). Given this aid dependence, exposure to development aid is likely to shape Sierra Leoneans' perception of white foreigners. However, the local experience of foreign aid has not always been positive. For example, Manning (2009) extensively documents accounts of broken promises by incompetent or fraudulent development practitioners.

A key institutional legacy of British indirect rule in Sierra Leone is the strong chieftaincy system. With the creation of the protectorate in 1898, the Paramount Chiefs—the highest-level traditional chiefs—were given substantial powers over their subjects (Fanthorpe, 2001; Jackson, 2006; Acemoglu et al, 2012). Chiefly power has been perceived to be so great, that some have attributed the civil war to local grievances generated from abuses of this power by the chiefs (Richards, 2005a; Mookuwa et al, 2011). After the war, there were attempts to reform the chieftaincy system and create more accountable, formal decentralized sources of power, including democratically elected district councils that set tax rates, and local magistrates that administer justice. However, *de facto* power still remains with the chiefs: "chiefs are regarded as legitimate traditional rulers, with broader political power bases than the council chairperson (Jackson, 2006)." They are still called upon to settle disputes (Sawyer 2005), and they retain the power to tax, request labour, administer justice, and allocate land. Nonetheless, after the war, many NGOs have preferred to operate through the new formal channels, rather than through village chiefs, due to concerns of corruption and a desire to strengthen formal governance

[3]The country ranked 180 out of 187 on the United Nation's Human Development Index in 2011, and 187 out of 187 in 2003.

structures (Jackson, 2006; Fanthorpe, 2001b). As a result, there is sometimes an adversarial relationship between chiefs and foreign aid workers, and chiefs may perceive foreigners as a potential threat to their power.

A second source of informal power in Sierra Leone is the secret societies: cultural institutions or "cult associations" (Richards, 2005b), which regulate social behavior through secret rituals and practices (Fanthorpe, 2007). Most Sierra Leoneans are members of secret societies[4] and its influence permeates all aspects of social and political life in Sierra Leone. A leadership position in one symbolizes power since these are highly hierarchical organizations. In fact, anthropologists such as Murphy (1980) argue that secret societies help perpetuate the strongly hierarchical, gerontocratic social structures in Sierra Leone. This is because power derives from one's position in a secret society; yet this position can only be obtained by seniority.[5] In most rural areas, the chief's powers are also intricately linked to the workings of the secret societies—in fact, Jackson (2006:105) notes: "It is also commonly asserted that critical governance issues have been decided by the secret societies rather than in the chiefdom councils."

3 Experimental design and empirical strategy

To causally identify the impact of white foreigner ('white-man') presence on measured generosity, we randomized the presence of a white 'supervisor' in the administration of a series of behavioral games. The experiment was carried out in 60 villages across five districts in Sierra Leone: Bombali, Kailahun, Koinadugu, Kono, and Moyamba (Figure 1). There are six treatment and six control villages in each district.[6] Figure 2 shows these treatment and control communities included in the study.[7]

All games were administered by a core team of Sierra Leoneans comprising one facilitator, two assistants, and a local translator recruited from the area where the games were being played. In addition, the team included a "supervisor" whose identity varied systematically across control and treatment villages. In the control villages, the supervisor was a Sierra Leonean from Freetown, the national capital. In the treatment villages, the supervisor was a white American. Both supervisors were chosen for similarity on key characteristics: male,

[4]In some communities, such as Kpelle society, all men and women are expected to be initiated into a secret society (Murphy, 1980).

[5]Murphy (1980) notes that "[S]ecrecy separates elders from youth. It supports the elders' political and economic control of the youth."

[6]These games were played as part of a larger randomized controlled trial of a community reconciliation program.

[7]We conducted the randomization over sections, which are geographic units that include several villages. However, prior to the randomization, we pre-selected one village in each section that was either the section headquarter or largest village in the section, for inclusion in the study. As such, each section selected corresponds to a single pre-selected village, and thus, in our case, the section level randomization is equivalent to village level randomization.

college-educated, and friendly demeanors.

Neither "supervisor" was made aware of the experiment being conducted in order to minimize the chance of expectancy effects (Rosenthal, 1978). At the same time, to prevent other differences between them (e.g. proactivity, competence, etc.) from confounding the 'white-man' treatment, all games were conducted according to a strict protocol and set script that specified a silent, background role for them. Their role was limited solely to taking notes and the distribution of money to players in the beginning of each game. They did not speak during the administration of the game or otherwise interact with game participants in any way and were not present in the room when subjects made their allocation decisions.

The 12 subjects who participated in the game session of each village were randomly sampled during a household survey conducted up to two months prior to the implementation of the games. If one of the originally sampled individuals could not be found, he or she was replaced by someone else from the same household, and of the same gender as the original target respondent. Replacement occurred for 7.5% of the sample.

Our measures of generosity were generated using three different versions of the standard dictator game. The advantage of the dictator game is that it provides a clean measure of other-regarding preferences, since payoffs to play are independent of beliefs on over other subjects' expected behavior.[8] In addition, the use of dictator games in field settings in developing countries has become increasingly popular: Figure 3 shows google scholar counts for the search terms "dictator games" and "Africa," and "dictator games" and "developing countries."

In each of our games, the subject was given 4,000 Leones (approximately $1), which is an amount slightly higher than the average daily income in Sierra Leone. The subject could decide how much of this money to keep and how much to 'give' to recipients.

The three games played were:

1. Game 1: Anonymous Own-village. This is the closest in form to the standard dictator game, played to provide a standard measure of generosity. The recipients were 12 additional individuals randomly sampled from the players' village. The players were told who these 12 recipients were, but not which recipient they would be matched to. Players were also informed that they would be re-matched to a different recipient in each game. Player contributions were strictly anonymous, i.e. no player was informed how much any other player had chosen to contribute.

2. Game 2: Non-anonymous Own-village. The purpose of this game was to develop a mea-

[8]We also conducted a trust game as part of the baseline for the reconciliation experiment. Contrary to the dictator game, the trust game incorporates a complex set of motivations. Subject play involves strategic interaction based on beliefs about the 'trustworthiness' of the other party, which may obscure experimenter demand effects. To prevent any element of strategic interaction from affecting play in dictator games, the trust game was conducted after all dictator games were completed.

sure of 'reputational concerns,' by comparing giving in Game 1, where contributions were private knowledge, to giving in a context where contributions were public knowledge. Thus we relaxed the between-subject anonymity condition from Game 1. Instead, players were told that their giving decision would be announced at the end of the session, after all games had been played. All other game elements remained the same. Recipients were the same individuals from Game 1, and as in that game, players were not told who they were matched to. To minimize feedback across games, the announcement was made in front of the subjects and recipients at the end of the session, after all games had been played. Recipients were not present during the game but arrived at the end after all games were played to receive their allocations.

3. Game 3: Anonymous Other-village. The purpose of this game was to develop a measure of 'out-group generosity,' by comparing giving in Game 1, where recipients were from the players' own village, to giving in a context where recipients were from another village. Players were told that we had identified 12 recipients in "another village" who would receive the amount they chose to give. All other game elements were the same as in Game 1 – no player was informed of any other player's contribution, who they were matched to, etc.

Games were conducted in two rooms, a public room where the group received instructions, and a private room where they individually entered to make their allocation decision. No talking was allowed during the explanation of the games, to prevent strategic interaction and framing by subjects before the games were played. To ensure that subjects understood the game, the facilitator repeated the explanation of the game to subjects in the private room, allowing them to ask questions. After the second explanation, the facilitator would exit the private room, allowing the subject to make her or his allocation decision privately. The subject was instructed to place the amount she or he wanted to give in an envelope, and place the envelop into the slit of a locked box.

To maximize subjects' comprehension, the games were also translated into seven local languages in three of the five districts.[9] To assure consistency between translations, each of these translations were back-translated to English. To account for the possibility that the teams may have improved in their communication skills over time, particularly in the translated districts, we varied the ordering of treatment and control villages, so that games were conducted first in treatment villages in half the districts, and first in control villages in the other half.

Based on this experimental design, the empirical strategy that we use to estimate the impact of the white foreigner effect is:

[9]The facilitator spoke the local languages in the other two districts.

$$y_{ivld} = \alpha_d + \delta_l + \beta(\textit{white-man}_{vld}) + \mathbf{Z}_{vld}\rho + \mathbf{X}_{ivld}\phi + \omega_{ivld} \quad (1)$$

where i is individual, v is village, l is majority language group, and d is district. y_{ivld} is the amount given in the dictator games; $\textit{white-man}_{vld}$ is an indicator of treatment to assignment; α_d are district fixed effects; and δ_l are majority language fixed effects. We include district fixed effects to account for substantial regional heterogeneity in economic development, urbanization, war exposure, and other factors that may influence generosity. We include majority language fixed effects to control for variation in underlying generosity levels across ethnic groups, since language groups correspond closely to ethnic/tribal affiliation in Sierra Leone. Majority language fixed effects also reduce noise created by the varying quality of communication across games, since some games did not need to be translated because the facilitator spoke the majority language, and since translator quality varied across translated games. $\mathbf{Z}_{vld}$ and $\mathbf{X}_{ivld}$ are additional village and individual level covariates included in some specifications. Here, β captures the white-man treatment effect.

In addition, when we pool the observations from the three different game types we estimate:

$$y_{givld} = \gamma_g + \alpha_d + \delta_l + \beta(\textit{white-man}_{vld}) + \mathbf{Z}_{vld}\rho + \mathbf{X}_{ivld}\phi + \omega_{givld} \quad (2)$$

where g denotes a game, and γ_g are game fixed effects. In all specifications, we cluster the standard errors at the village level.

4 Data

The behavioral games data used in the analysis is supplemented by household and village level survey data that was collected prior to implementation of the games.[10] This provides us with a rich data on a number of different characteristics of the players and the villages in which they reside. We have village level data for the 60 villages included in our sample. We have household survey data for 715 out of 720 participants, though the sample size varies for particular measures.

To account for ethnicity, we construct an indicator variable for each language spoken by the majority of the respondents in each village. Ethnicity is geographically concentrated and identified by language. The language spoken by the majority of villagers therefore provides a measure of the majority ethnic group in the village. It also serves the additional function

[10]This village and household survey data were collected as a part of the baseline for the reconciliation experiment.

of accounting for heterogeneity in translator quality, as discussed above. There were seven ethnic groups among our sampled villages (Fullah, Kono, Koranko, Loko, Mende, Temne and Yalunka). However, there was only one Yalunka majority village, and thus, for robustness, we constructed two additional measures of ethnicity using broader linguistic parent groups from the third and fourth levels in the Atlantic language hierarchy.[11]

Given Sierra Leone's recent conflict, we control for violence related effects by using a measure of the number of buildings burnt in each village during the war. We also measure village size (based on the number of households residing in the village) and level of market integration, with an indicator for whether there is a market in the village. In addition, we use two measures that proxy for village cooperation: an indicator of whether the village has communal farms and an indicator of whether it has a labor gang, which refers to a group of people who work on each others' farms.

After the end of its civil war, Sierra Leone received a large amount of post-conflict development assistance from donor nations, making it one of the most aid-dependent nations in the world. To measure each village's exposure to foreign aid, we look at NGO involvement in the provision of local public goods.[12] To account for varying degree's of NGO involvement, we construct several different variables. The broadest indicator is based on whether a NGO either owns a school or clinic, or currently provides resources to the local school or clinic, or has contributed to the construction of public facilities (including schools, clinics, wells, latrines, or the palava hut[13]).[14] This variable is called "NGO aid," and 54 villages in our sample (all but six) have some form of aid engagement under this metric.

We posit that perceptions of foreigners are likely to be mediated based on the extent of aid exposure, and thus construct two continuous measures capturing the length of aid exposure. The first variable, "Years of aid with NGO-owned facility," refers to the number of years that a NGO has owned either a school or clinic in the village. It is worth noting that NGOs own these public facilities in 31 (or just over half) the sample villages. As an additional measure, we construct "Years of aid with NGO activity," which is the number of years a NGO has either owned a school or a clinic; or the number of years since a NGO contributed to the construction of the school or clinic, if a NGO provides current support to the school or clinic. Although this variable has the advantage of being more inclusive (capturing NGO activity in 5 additional villages where they do not own facilities), we cannot verify whether these organizations engaged continuously over the period between their initial contribution and current support, and thus view it as an

[11]The Atlantic language hierarchy is catalogued in http://multitree.org.

[12]In the Sierra Leonean context, nearly all large NGOs involved in providing public goods in rural villages are foreign or have foreign funders.

[13]The palava hut is often the only public space in the village for community members to congregate.

[14]A large component of post-conflict aid in Sierra Leone was for 'reconstruction', which focused on infrastructure development (Fanthorpe, 2001)

auxiliary measure. Finally, our household survey tracks whether any white foreigners came to the village in the past year for the purpose of aid provision ("White aid visitor").We also employ an indicator of whether villages had non-aid white visitors only ("White non-aid visitor").

To measure household socioeconomic status, we develop an asset index. As is standard in the literature (Vyas 2006), we use principal component analysis to construct an aggregate measure based on ownership of a broad range of assets, the material of the roof and floor of the house in which the respondent lives, as well as ownership of land.[15] In addition, we have data on the respondent's educational attainment, which is used to generate an indicator of whether he or she has some formal education. We are also able to examine the player's past exposure to white foreigners, since the household survey asked how often the respondent has met a white person before. We additionally ask the respondent whether he/she is the village chief; and if someone in their household is the leader of a secret society.

Finally, after the games were conducted, we asked the participants to choose what they believed to be the purpose of the research team's visit. Specifically, the participants were asked: *"Why do you think the researchers were playing these games? Choose the statement you agree with most."* The following three statements were then read out to them: "To give money to the community in a fun and educational way"; "To test the community, to see which community is more deserving of aid"; and "To find out more about how members in this community think about each other, interact with each other, and treat each other." The last option corresponds most closely to the stated purpose of the research team's visit prior to the start of the games, which was to find out about the community. In the text and tables, we refer to the first statement as "Give money", the second as "Aid test" and the third as "Research."

4.1 Descriptive statistics

Table 1 presents the descriptive statistics of the key independent and dependent variables used in the analysis. We utilize four key dependent variables from the behavioral games: total giving, which is the sum of giving from the three dictator games; as well as separate giving in each of the dictator games respectively: the anonymous own village game, the anonymous other village game, and the non-anonymous own village game.[16] When we pool across all three games, we refer to the dependent variable as "Giving". As shown in Table 1, the average total giving was 3216 leones out of a total of 12000 given to players in the three games. The average mean giving across the three games was 1079.

[15]We cannot use measures such as income or wages to control for wealth since the majority of respondents are subsistence farmers.

[16]The anonymous other village game is only available for 708/720 observations owing to enumeration error, since the values for this game were not recorded in one of the villages.

A number of our variables demonstrate the depth of foreign aid engagement in Sierra Leone. The average years of aid exposure based on NGO ownership of a school or clinic is 12 years. The "White aid visitor" variable shows that approximately 36% of our sample resided in a village that received one or more white aid visitors in the past year. In addition, the foreign exposure variables indicate that while 10% of our sample has never met a white person before, 46% has met a white person between one and 10 times (while the remaining 44% has met a white person more than 10 times). In terms of the belief variables, 64% of the participants chose the "Give money" option, which is unsurprising given the payouts made during the games. Another 22% chose the "Research" option, while the remaining 14% opted for the "Aid test" choice.

Table 2 examines key characteristics in the control and treatment groups, including village and individual characteristics. Column (1) shows the control group mean, column (2) shows the treatment group mean. Column (3) shows that there are no statistically significant differences in these characteristics across the two groups with the exception of respondent age. Though this difference is small and only significant at the 10 percent level, below, we show that our results are robust to controlling for age.

We begin a with a simple graphical exploration of whether the white-man treatment induced differential giving. Figure 4 examines the mean of total giving in the control group (2943.966) and the treatment group (3480.833). The statistical significance bars display pair-wise statistical significance based on standard errors clustered at the village level, and show that the difference of approximately 537 leones is statistically significant, at the 10% level. Figure 5 shows the CDF of this variable in treatment and control regions. The figure shows that the treatment distribution first-order stochastically dominates the control distribution: a higher proportion of the treatment group gave more, evaluated at each level of giving, indicating that the white-man treatment induced an upward shift in the distribution of giving in the dictator games.

5 Results

5.1 White-man presence and measured generosity

In this section, we examine the impact of white-man treatment on giving in the dictator games, using regression analysis. We build on the simple means shown in Figure 3, by estimating equation (1). The odd numbered columns in Table 3 show specifications without ethnicity fixed effects, and the even numbered columns present equivalent specifications including ethnicity fixed effects, which improve the precision of the estimates. The coefficients in columns (1) and (2) show that the white-man treatment significantly increased the sum of total giving in the three dictator games. With ethnicity fixed effects, giving increases by 564 leones. This represents a 19 percent increase in average total giving, relative to the control mean (2944

leones). In the Appendix Table, we show the robustness of this substantial effect to the inclusion of respondent age as a control, which improves both the precision and size of the effect. This table also shows that the main effect is robust to the inclusion of our alternative ethnicity fixed effects, generated on the basis of broader linguistic categories.

Columns (3)-(8) show these effects separately for each of the three game types. The effect is most precisely estimated for the anonymous own-village game, where the coefficient is significant at the 5 percent level. The coefficient is marginally insignificant (with a p-value of .101) for the anonymous other-village game. The magnitude of the coefficients are also largest in the these two games, where the implied effects are 23 and 21 percent, respectively. In contrast, the coefficient on the non-anonymous own village game is both smaller in magnitude and less precisely estimated.

To further explore how giving varies across the games, in Table 4, we estimate equation (2) which pools giving in all three games. Here, all specifications include game fixed effects in which the anonymous own-village game serves as the omitted category. Column (1) presents the mean effect. The coefficient of 174.003 indicates that giving was 17 percent higher in treatment villages relative to the control group mean (of 997.191), and this difference is statistically significant.

The coefficients on the two game indicator variables are instructive for understanding basic differences in giving across the different types of games. The coefficient on the Other village game shows that individuals give significantly less when the proceeds go to an outside village, relative to their own village. Members thus prefer to give to their own group relative to an out group. In contrast, the coefficient on the Non-anonymous own village indicator shows that giving is significantly higher when proceeds are given non-anonymously, relative to anonymously, in the players' own village. This is consistent with previous findings that reducing anonymity among subjects increases giving**,** and underscores the idea that reputational concerns lead individuals to demonstrate greater generosity when allocations are publicly reported.

To see if the white-man treatment effect significantly differs across games, column (2) presents specifications where we interact each of these game types with the treatment dummy. Here, the coefficient on white-man gives the treatment effect for the anonymous own village game. The interaction of white-man with the other village indicator (-48.818) is statistically insignificant, indicating that individuals do not respond differentially to the treatment in own-village versus other-village giving. In contrast, the coefficient on the interaction of white-man and the non-anonymous indicator shows that the treatment effect becomes significantly smaller when non-anonymity is introduced. This suggests that measured generosity is higher in the presence of the white foreigner because players are concerned about his perceptions regarding their allocation, and this reputational concern vis-à-vis the foreigner is eroded by reputational concerns vis-à-vis other members of their own community.

In summary, these results present robust evidence that the mere presence of a white foreigner on a team of five researchers increases measured total giving in the dictator games by 19 percent, with largest increases in the anonymous own-village setting, where giving increased by as much as 23 percent. Documenting this significant and substantial treatment effect is the main finding of our paper. However, in the sub-sections below, we draw on our household and village survey data to present suggestive evidence on the mechanisms through which white foreigner presence may affect measured generosity.

5.2 Mechanism: Authority and Power Differentials

First, we examine the role of power differentials between the foreign researcher and local subjects in driving higher giving. It is plausible that the desire to impress the white foreigner may manifest itself as an "experimenter demand" effect (Zizzo, 2008), where subject behavior is driven by a desire to "please" the experimenter. According to Loewenstein (1999), "[i]n social encounters, including laboratory experiments, most are engaged in a constant search for cues about how they are supposed to behave." Players infer that the experimenter wants them to display more generosity in the dictator games, and then act accordingly. This motivation could stem from deference to someone perceived to be in a position of authority. For example, Schultz (1967) describes the lab as a "superior-subordinate" relationship matched only by that of "parent and child, physician and patient, or drill sergeant and trainee (as quoted in Levitt and List, 2007)."

While power differentials may exist between experimenter and subjects in both treatment and control areas, we posit that in the Sierra Leonean context, this power differential is magnified with respect to the white foreigner. First, there are stark disparities in wealth between locals and white visitors to rural villages in the country.[17] In fact, most interactions between these two groups in rural areas are based on foreigners wielding influence over locals: foreign aid workers dispensing aid; UN peacekeepers providing security; international organizations restructuring governance; and business and miners buying and potentially exploiting land.

If the white-man treatment operates by reinforcing power differentials, the magnitude of the treatment effect should differ across subjects based on their position of power and relative authority within their communities. To test this hypothesis, Table 5 examines heterogenous effects across two groups that symbolize power and authority in Sierra Leone: chiefs and leaders of secret societies. Chiefs are either leaders of their village, or larger geographic areas called chiefdoms, and are ultimate arbiters of power in the informal authority system. The leaders of secret societies (and those that come from the same ruling lineage) have great informal power: most Sierra Leoneans are members of a secret society and these societies serve to regulate social

[17] Anecdotally, the arrival of a white visitor in remote rural villages is often treated with reverence.

behavior and preserve social structure.

From the household survey, we are able to identify whether our respondent is a chief, or comes from a household of a secret society head. Since household level characteristics are not available for replacements, column (1) begins by showing that the average treatment effect continues to hold when the sample is restricted to non-replacements[18] for who we have respones on the leadership variables.

Column (2) of Table 5 interacts the white-man treatment with the chief indicator. The negative coefficient on the interaction term shows that the treatment effect is 48% smaller for chiefs relative to the chief-specific control group mean of 4125. In fact, although chiefs give more relative to others in control areas, the average treatment effect (obtained by summing the coefficient across the White-man and interaction terms) is negative for this sub-group: chiefs actually give 1364 (or 33%) *less* in treated villages relative to control villages. Column (3) shows the same pattern of results hold for individuals who come from households of secret society heads.[19] These individuals are also found to give more relative to others in control areas, but display a negative treatment effect of 2486 (or 88%) less giving in the presence of the white researcher, relative to their control group mean.

These heterogenous effects suggest that the relative power differential between subject and experimenter plays a role in mediating the treatment effect: non-leaders respond positively and give more with the white-man treatment, relative to leaders. This is consistent with the idea that power differentials contribute to the treatment effect by magnifying the experimenter demand effect.

In addition, the robust negative effects associated with leaders suggest that arbiters of local authority may counter-react to what they perceive as an implicit demand to give to others, levied by an individual viewed as a foreign authority figure within the experimental context. This may reflect potentially adversarial relationship between informal sources of power in villages and foreigners in the post-war period. Foreign aid workers often prefer to operate through the new formal local government structures, such as local councils, out of both a desire to strength formal institutions and a perception of corruption by chiefs (Jackson, 2006; Fanthorpe, 2001). Village chiefs may therefore view foreigners as a threat to their power and be less deferential

[18]Although the number of replacements is distributed fairly evenly across treatment (30) and control (24) villages, if the type of individual serving as a replacement varies across these control and treatment areas, this could bias estimated effects. However, the third column of the Appendix Table shows that the effects are robust to controlling for a replacement indicator, and the coefficient on this variable shows that overall, replacements do not display significantly different levels of measured generosity. Moreover, the fourth column also shows that the magnitude of the treatment effect does not vary significantly by replacements. These results, combined with the robustness of the main effect excluding replacements in column (1) of Table 5, suggest that replacements do not vary systematically, and their exclusion does not systematically alter our estimated effects.

[19]Our household survey data ask whether some household member is a secret society leader. While this does not enable us to dicern if the person himself or herself is the leader, it enhances the likelihood of truthful reporting relative to asking the question directly of the respondent

to their authority.

One potential concern with these estimates is that there are a small number of leaders and authority figures, with 11 chiefs and 34 individuals from households with secret society heads appearing in our sample. To show that the estimated effects are not driven by any one individual, we re-estimate the specifications in columns (2) and (3), leaving out each of the leaders. Figure 6 plots the t-statistics associated with the interaction term from each of these regressions, for chiefs in the top graph and secret society heads in the bottom graph. The t-statistcs do not fall below 1.64 in absolute value terms, demonstrating that the effect remains significant throughout all regressions.

Next we address the concern that other demographic characteristics correlated with influence are driving these effects. Influential individuals tend to be older, male, relatively wealthy, and are members of the majority ethnic group of the village. In column (5) of Table 5, we control for age, gender, and the majority ethnic indicator, as well as a household assets index and an indicator of whether he or she has any formal education as a proxies for wealth. We also interact all these controls with the treatment, and the results remain significant. Since we lose sample size with the addition of individual level controls (due to missing observations), we also show in column (4) that the average treatment effect remains significant in the reduced sample.

Finally, it is possible that leaders may be reacting differently due to greater past interaction with white persons relative to the rest of the community. This may reduce experimenter demand effects by mitigating how foreign or novel the white researcher seems. Thus column (5) also controls for two variables (never met a white person before, and met a white person between one and 10 times, both defined relative to the omitted category of met a white person more than 10 times). The chief and secret society interactions remain significant with the inclusion of these control interactions, indicating that past exposure to white foreigners cannot account fully for these effects. It also suggests that differential giving in these sub-groups reflects dynamics that emerge in the experimental setting itself, related to factors such as authority.

5.3 Mechanism: the Role of Aid Exposure

Given Sierra Leone's historical exposure to foreign aid, it is possible that citizens generally perceive white visitors to be associated with aid disbursement. To test if these perceptions affect subject behavior, we assess whether players' response to the white-man treatment varies based on their community's past exposure to development aid.

We begin by outlining two potential channels through which measured generosity in the presence of the white researcher could vary based on previous aid experience:

1. **Altruism-signalling:** Aid-exposed participants may display relatively more generosity in dictator games to signal that they are altruistic and cooperative, and therefore more

deserving of aid. Under this account, more aid-exposed participants should (a) be more likely to believe that the white researcher is there to test the community to determine their future aid allocation and (b) give relatively more during dictator games played in his presence.

2. **Need-signalling**: Aid-exposed subjects may display less generosity to strategically signal that they are poor and thus in need of development assistance. Under this account, more aid-exposed participants should (a) be more likely to believe that the white researcher is there to test the community regarding future aid, but (b) give relatively *less* when the game is played in the presence of the white researcher.

To test these hypotheses, we examine if there are differential effects of the white-man treatment based on past aid exposure on two sets of dependent variables: measured generosity in the dictator games, as well as participant beliefs regarding the purpose of the games.

5.3.1 Aid exposure, White-man Presence and Generosity

We begin by examining the effect of exposure to aid on giving. Table 6 reports the results from interacting the white-man treatment with the "Years of aid with NGO-owned facility" variable.[20] The coefficient on the white-man base term in column (1) is 751. This indicates that communities without a NGO-owned school or clinic (approximately half the sample villages) give 32% more in the presence of the white researcher (above the relevant control group mean of 2,320). However, the interaction term is negative and significant, suggesting an erosion of this positive treatment effect among communities that have had longer exposure to aid. For example, at the mean exposure of approximately 12 years, the white-man effect is to give 510 more, which represents a 10% drop in the magnitude of the estimated effect relative to no aid exposure.

It is possible that communities respond differently to the treatment, not because of aid experience, but because villages receiving aid for longer periods may have other characteristics which determine giving. For example, these communities might be larger, less integrated, better organized, richer, and have more exposure to white foreigners in general. To account for these potential omitted factors, we incorporate a number of individual and village-level controls in column (3) of Table 6. We include treatment interactions with village size and whether the village has a market, which are both important indicators of economic development and the degree of market integration. To control for underlying societal cooperation, we introduce

[20] As discussed in Section 4, this variable refers to the number of years that development NGOs have owned either a school or a clinic in the facility. This metric is our main measure of aid exposure, since ownership generally corresponds to sustained NGO engagement over the duration since the facility's initial construction.

indicators of whether the village has communal farms or a labor gang. We also control for the number of buildings burnt during the civil war, as post-conflict aid may have been targeted to areas with higher levels of historical violence. To account for wealth, we include the individual level control interactions from column (5) of Table 5. In addition, to address the concern that it is general exposure to white persons, rather than the aid experience itself driving differential giving, we continue to control for the white foreigner exposure variables. Column (2) shows the robustness of the average treatment effect to the subset of observations for which all control variables are defined. Column (3) shows that the aid interaction remains significant with the full set of individual and village-level controls. This column also shows that the chief and secret society interactions remain significant after we include the full control set.

In Table 7, we build on the aid exposure account with several alternative measures of NGO activity. Although ownership of schools and clinics corresponds to sustained engagement, this metric omits other possible forms of NGO involvement, including contributions to other types of public goods such as wells or latrines. Thus, we look at equivalent interactions of the white-man treatment with the "NGO aid" indicator — our broadest measure — which suggests that NGOs have been involved with public goods provision in all but six sample villages at some point in time. In column (1), the negative coefficient on the interaction term (-1995.59) again shows that the treatment effect is substantially smaller in communities with some previous aid exposure, by approximately 66% compared to the relevant control group mean. Column (2) of Table 7 reports similar results using a third measure of exposure: "Years of aid with NGO activity." This underscores the robustness of the finding that places with greater aid exposure respond less to the white-man treatment.

To what extent can we assert that these differential effects reflect the village's exposure to foreign aid specifically, rather than other types of white foreigners? Column (3) of Table 7 introduces the interaction of the treatment with an indicator of whether any white visitors came to the village for the purpose of providing aid over the past year.[21] This interaction term is also negative and significant (implying that the effect is 36% smaller for places that received white aid visitors). In contrast, column (4) shows that the equivalent interaction involving an indicator of whether the village had only non-aid white visitors is positive and insignificant.

In summary, Tables 6 and 7 have shown that the white-man treatment is significant and positive among communities with no NGO aid engagement, and the extent of increased giving decreases with aid exposure. This provides suggestive evidence favoring the "need-signalling" channel, rather than the "altruism-signalling" channel, since respondents from communities with more aid exposure give relatively *less* under the treatment.

Two additional points are worth noting regarding the interpretation of this effect. Firstly,

[21]While the ideal data would track different types of white visitors over a longer period, recall bias poses a challenge in collecting such data going further back in time.

aid-exposed communities, on average, still give more in the presence of the white foreigner. Thus, the overall pattern of results is consistent with the idea that "experimenter demand effects" are the key mechanism behind participant giving, but this effect is dampened among players who strategically give less out of a desire to secure future aid. Secondly, although the aid exposure result counters the notion that aid experience leads players to give more to get more (as suggested by altruism-signalling), as a caveat, it is also consistent with several different explanations of why aid exposure leads to a lower response. For example, aid-exposed participants may display less generosity in the presence of the white researcher because they have come to associate white foreigners with handing out money, and perceptions of foreign generosity may "crowd out" local generosity. The results are also consistent with an "aid fatigue" argument, in which the experimenter demand effect diminishes in communities with greater aid experience. Under this account, the desire to respond to perceived experimenter demands may be smaller in areas where villagers have tired of responding to demands levied on them by foreign aid workers. While we cannot rule out these other channels, a key prediction of the "need-signalling" account is that more aid-exposed participants associate the white researcher with testing them for aid. To examine this prediction, we next turn to data on perceived beliefs regarding the purpose of the games.

5.3.2 Aid exposure, White-man Presence and Participant Beliefs

In this sub-section, we examine responses from a question designed to gauge respondent beliefs regarding the purpose of the behavioral games. As discussed in Section 4, after the games were completed, subjects were asked why they thought the researchers had played the games, and were given the "Aid Test," "Give Money" and "Research" options. If the Need-signalling hypothesis holds, then subjects more exposed to aid should disproportionately choose the aid test option in the treatment group, since, according to this account, respondents strategically give less in the presence of the white man since they believe he is there to determine if the community will receive aid.

First, we examine if overall respondent choice varies significantly based on the white-man treatment. Column (1) of Table 8 uses a simple logit model where the dependent variable equals one if the respondent chooses "Aid test" and zero otherwise. We find a significant treatment effect of 0.439, equivalent to 56.1% lower odds of choosing this option in the presence of the white researcher. However, since this logit specification groups together the money and research options, we next employ a multinomial logistic model, using a categorical dependent variable of participant belief that includes all three choices. Columns (2)-(4) show the relative coefficients for each option pair: "Aid test" over "Research," "Aid test" over "Give money" and "Give money" over "Research." The results are consistent with those in the first column:

in the presence of the white man–the relative risk ratio (the equivalent of the odds ratio) of choosing "Aid test" is 54.5% lower than choosing "Give money," and 60.8% lower than choosing "Research." In contrast, the treatment makes no significant difference in the choice between "Give money" and "Research". This finding suggests that in the presence of the white researcher, subjects were more likely to believe the stated purpose of the team's visit (to find out about the community), or to view what the team did (give out money) as the purpose of the game, rather than to second-guess that their agenda was a test for aid.

Next, we assess if there is variation in players' choice based on interaction between the white-man treatment and past aid exposure, as measured by years of aid with NGO-owned facilities. The results indicate that, while communities with no exposure are less likely to choose the aid-test option, this effect is countered with longer aid exposure. Column (1) presents the logit on "Aid test." The coefficient on the white-man term implies that people living in communities with no aid exposure have a 72.9% lower odds of believing that the purpose of the game is to test people for aid in the presence of the white researcher.[22] However, every year of aid erodes this effect by 5.2%. Column (2) shows robustness to the addition of our full set of individual and village level control interactions. Columns (3)-(8) indicate similar results with the equivalent multinomial specification: each year of aid exposure increases the relative risk of choosing "Aid test" over "Give money" by 5.2%, and over "Research" by 5.5%, in treatment versus control areas. Again, the associated relative risk of choosing the research versus money options are not significantly different.

Table 10 shows the same pattern of results with two alternative measures of aid exposure ("NGO aid" and "Years of aid with NGO activity"), as well as the white aid visitors measure. These results reiterate that communities receiving aid over a longer period were differentially more inclined to believe that the research team including a white foreigner was there to test the community for aid.

It is also worth noting that the chief and secret society interactions are insignificant in Tables 9 and 10, indicating that there are no disproportionate effects on how the white researcher's presence shapes their beliefs regarding the purpose of the games. In other words, the differential giving of these authority figures under the white man treatment is not due to perceptions regarding aid.

In summary, Tables 9 and 10 provide further supportive evidence for the "Need-signalling" channel: in communities with more aid exposure, participants were more likely to believe that the purpose of the game was to test the community for aid in the presence of the white researcher. This is consistent with the idea that past experience with development assistance inclines players to adapt a more strategic approach of giving less in the game, with the aim of

[22]This is equivalent to going from, for example, an odds ratio of 1.0 (50:50) to an odds ratio of 0.27 (21:79), or a 29% decrease in probability.

getting more resources in the future.

While these findings provide suggestive evidence of "Need-signalling," it is important to reiterate that they do not conclusively rule out other potential channels. It is likely that a complex set of motivations guide how subjects respond to the white foreigner in the context of behavioral games, and future work should aim to experimentally distinguish across various aid-related channels.

6 Conclusion

This paper has examined whether researcher identity can affect measured generosity in a developing country context, by drawing on the results of a field experiment which varied the presence of a white foreigner in the administration of 60 behavioral games in rural Sierra Leone. We have found that the mere presence of the white foreigner on a research team of five increases total giving in dictator games by 19 percent. In our experimental design, one member of the research team was designated to be a "supervisor," who was a Sierra Leonean in control communities, and a white foreigner in treatment areas. The experimental protocol specified a silent and limited role for this team member, whose sole responsibility was to distribute money, and who was not allowed to speak with players during the games. Nonetheless, this change in the composition of the research team induced differential giving among the randomly sampled players from the villages in which the games were conducted.

The participants played three different dictator games – the anonymous own village game, the anonymous other village game, and the non-anonymous own village game – which allows us to examine which type of giving elicited the largest responses. We find that the 'white-man' treatment effect was largest in anonymous own village game, where giving was 23 percent higher in the treatment group relative to the control group. In contrast, the treatment effect was statistically insignificant and smaller in the non-anonymous game, where allocations were reported publicly, at the end of all the games. We interpret this as evidence that reputational concerns regarding members of one's own community, which are introduced with non-anonymity, crowd out potential reputational concerns regarding the white foreigner.

By linking the games data to household and village level survey data, we present suggestive evidence on potential mechanisms. This analysis indicates that the generosity effects may reflect power differentials between white foreigners and local Sierra Leoneans, which heighten experimenter demand effects. As evidence, we find substantial authority-based heterogeneity in the treatment effect: while non-leaders give more, leaders in powerful positions, particularly chiefs and members of households leading secret societies, give *less* in the presence of the white researcher. This is consistent with lower deference and counter-reaction by local authority figures to outside sources of authority.

In addition, our findings indicate that the association of white foreigners with development assistance may lead to strategic giving. Subjects from more aid-exposed villages are more likely to believe that the purpose of the game is to test the community for aid allocation, in the presence of the white researcher. Since these subjects also display lower measured generosity in response to the white-man treatment, the results together suggest that they give less to signal poverty and the need for future aid.

Our study also holds direct implications for how we interpret current measures of generosity and for evaluations that utilize behavioral games in developing countries. It shows that measures of generosity may be upward biased relative to underlying other-regarding preferences, depending on who is carrying out the measurement. In addition, if there is heterogeneity in how subjects respond to the foreign researchers, the finding creates an additional challenge in interpreting variation in cross-cultural or cross-national measures of generosity. For example, Henrich et al. (2006) find large variation in measured generosity across developing countries and attribute this to differences in cultural practices. However, if communities vary in their response to foreigners based on factors such as previous exposure to aid, then the results may be driven in part by different responses to foreign researchers rather than cultural differences *per se*. Finally, our results hold implications for impact evaluations of aid interventions, which are increasingly combined with behavioral games (for example, see Fearon et al., 2009). Our finding suggests that if research teams are not balanced in terms of their racial and national composition across treatment and control areas, this could generate potential bias in the evaluation results. Overall, by demonstrating how the presence of a foreign researcher can affect measures of local generosity, the analysis points to the import of considering researcher identity broadly in measuring and interpreting behavioral outcomes.

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Table 1. Summary statistics

	Obs.	Mean	Std. Dev.	Min.	Max.
Total giving	708	3216.949	2648.112	0	12000
Giving in anonymous own-village game	720	1065.278	1032.121	0	4000
Giving in non-anonymous own-village game	720	1211.250	995.595	0	4000
Giving in anonymous other-village game	708	959.040	1005.158	0	4000
Giving	2148	1079.190	1015.905	0	4000
Belief aid test	719	0.139	0.346	0	1
Belief give money	719	0.641	0.480	0	1
Belief research	719	0.220	0.414	0	1
Chief	708	0.017	0.129	0	1
Secret society leader	720	0.050	0.218	0	1
Years of aid with NGO-owned facility	720	12.183	22.499	0	106
Years of aid with NGO activity	720	13.417	22.406	0	106
NGO aid	720	0.900	0.300	0	1
White aid visitor	708	0.356	0.479	0	1
White non-aid visitor	708	0.390	0.488	0	1
Met white person 1 to 10 times	715	0.457	0.499	0	1
Never met white person	715	0.102	0.303	0	1
Female	715	0.543	0.499	0	1
Ethnic majority	714	0.894	0.309	0	1
Age	712	42.329	15.383	12	110
No formal education	711	0.612	0.488	0	1
Household asset index	706	0.000	1.971	-1.72	20.34
Number of households in village	720	284.267	411.020	3	2000
Village has a market	720	0.133	0.340	0	1
Buildings burned during war	720	5.767	15.429	0	100
Village has a labor gang	720	0.950	0.218	0	1
Village has a communal farm	720	0.367	0.482	0	1

Notes. Total giving is the sum of giving in the anonymous own village, anonymous other village, and non-anonymous own village games. Giving is the amount given across the three dictator games when the observations from the games are pooled together. Belief aid test, give money and research are indicators for whether participants believed that researchers wanted to test for aid, distribute money, or find out more about the community, respectively. Chief indicates that the respondent is a chief. Secret society leader indicates that the respondent is from the household of a secret society leader.

Table 2. Key individual and village-level characteristics

	(1) Treatment	(2) Control	(3) Difference	(4) Std. Err.
Individual-level characteristics				
Chief	0.011	0.023	-0.011	(0.008)
Secret society leader	0.039	0.061	-0.022	(0.018)
Met white person 1 to 10 times	0.437	0.478	-0.039	(0.048)
Never met white person	0.120	0.084	0.038	(0.032)
Female	0.546	0.539	0.007	(0.039)
Ethnic majority	0.874	0.913	-0.039	(0.034)
Age	41.225	43.426	-2.228*	(1.172)
No formal education	0.593	0.631	-0.038	(0.044)
Household asset index	0.003	-0.003	-0.003	(0.232)
Village-level characteristics				
Years of aid with NGO-owned facility	10.133	14.233	-4.100	(5.633)
NGO aid	0.933	0.867	0.067	(0.076)
White aid visitor	0.345	0.367	-0.012	(0.116)
White non-aid visitor	0.414	0.367	0.046	(0.127)
Years of aid with NGO activity	11.133	15.700	-4.567	(5.570)
Number of households in village	334.233	234.300	99.933	(94.736)
Village has a market	0.167	0.100	0.067	(0.087)
Buildings burned during war	6.100	5.433	0.667	(3.559)
Village has a labor gang	0.933	0.967	-0.033	(0.054)
Village has a communal farm	0.267	0.467	-0.200*	(0.106)
Dominant ethnic group				
Fullah	0.033	0.033	-0.000	(0.044)
Kono	0.200	0.200	-0.000	(0.000)
Koranko	0.167	0.133	-0.033	(0.050)
Loko	0.067	0.067	-0.000	(0.055)
Mende	0.400	0.400	0.000	(0.000)
Temne	0.133	0.133	-0.000	(0.055)
Yalunka	0.000	0.033	0.033	(0.032)

Notes. Columns 1-2 provide the means of each variable in the treatment and control groups, respectively. Column 3 displays the coefficients from a regression of each variable on the white-man treatment dummy. Column 4 displays the robust standard errors, clustered at the village level. * is significant at the 10% level.

Table 3. White man presence and measured generosity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total giving		Anonymous own village giving		Non-anonymous own village giving		Anonymous other village giving	
White-man	537.320*	564.943*	219.444**	223.585**	103.056	118.994	173.740	183.908
	(312.028)	(296.734)	(107.248)	(102.443)	(105.064)	(103.127)	(117.578)	(110.449)
Control group mean	2,943.966	2,943.966	955.556	955.556	1,159.722	1,159.722	872.126	872.126
Ethnicity fixed effects	N	Y	N	Y	N	Y	N	Y
Observations	708	708	720	720	720	720	708	708

Notes. Robust standard errors clustered at the village level in parentheses. All specifications include district fixed effects. In column 1 and 2, total giving is the sum of giving in the Anonymous own village, Anonymous other village and Non-anonymous own village games. ** is significant at the 5% level and * is significant at the 10% level.

Table 4. White-man presence, anonymity and other-village giving

	(1) Giving	(2) Giving
White-man	174.003*	229.105**
	(98.905)	(104.374)
White-man x non-anonymous own village game	-	-116.389*
	-	(68.960)
White-man x anonymous other village game	-	-48.818
	-	(67.544)
Non-anonymous own village game	145.972***	204.167***
	(35.291)	(37.073)
Anonymous other village game	-107.481***	-83.127*
	(34.041)	(44.766)
Control group means:		
All games	997.191	-
Anonymous own village game	-	955.556
Non-anonymous own village game	-	1159.722
Anonymous other village game	-	872.1264
Observations	2,148	2,148

Notes. Robust standard errors clustered at the village level in parentheses. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 5. Mechanism: White-man presence, power and generosity

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving	(6) Total giving
White-man	564.189*	604.135**	715.126**	577.002**	414.462	610.390
	(284.512)	(291.054)	(285.515)	(283.976)	(1,106.126)	(1,512.031)
Chief x white-man		-1,968.604**			-2,130.588***	-2,368.161**
		(800.533)			(779.438)	(966.220)
Chief		1,571.898***			955.727	766.215
		(500.387)			(582.937)	(632.942)
Secret society leader x white-man			-3,201.169***		-3,055.279***	3,283.751***
			(869.852)		(901.905)	(898.765)
Secret society leader			1,476.676**		1,188.501	1,246.219*
			(712.740)		(724.842)	(713.574)
Met 1 to 10 white people x white-man					361.787	227.092
					(500.680)	(506.344)
Met 1 to 10 white people					-30.572	27.308
					(245.677)	(641.684)
Never met white person x white-man					159.817	-39.530
					(664.069)	(240.044)
Never met white person					-123.609	-289.657
					(428.426)	(421.954)
Individual-level controls	N	N	N	N	Y	Y
Village-level controls	N	N	N	N	N	Y
Observations	644	644	653	630	630	630

Notes. Robust standard errors clustered at the village level in parentheses. Chief indicates that the respondent is a chief. Secret society leader indicates that the respondent is from the household of a secret society leader. Met white person 1 to 10 times and never met white person are indicators of the respondent's past exposure to white persons. Other individual-level controls include respondent age, ethnic majority group indicator, formal education indicator, a household asset index, and their respective interactions with the white-man variable. Village-level controls include village size, buildings burned during the war, indicators for whether there is a market, a labor gang, and a communal farm, and their interactions with the white-man treatment. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 6. Mechanism: White-man presence, aid exposure and generosity

	(1) Total giving	(2) Total giving	(3) Total giving
White-man	751.564**	577.002**	926.952
	(319.051)	(283.976)	(1,392.476)
Years aid NGO-owned facilities x white-man	-20.157**		-20.023**
	(9.540)		(8.400)
Years aid NGO-owned facilities	6.150		3.051
	(6.971)		(5.538)
Chief x white-man			-2,411.087**
			(982.397)
Chief			707.166
			(621.954)
Secret society leader x white-man			-3,281.101***
			(870.478)
Secret society leader			1,193.186*
			(695.823)
Met 1 to 10 white people x white-man			228.060
			(510.666)
Met 1 to 10 white people			-42.507
			(240.807)
Never met white person x white-man			64.007
			(637.577)
Never met white person			-278.336
			(422.458)
Individual-level controls	N	N	Y
Village-level controls	N	N	Y
Observations	653	630	630

Notes. Robust standard errors clustered at the village level in parentheses. Chief indicates that the respondent is a chief. Secret society leader indicates that the respondent is from the household of a secret society leader. Met white person 1 to 10 times and never met white person are indicators of the respondent's past exposure to white persons. Other individual-level controls include respondent age, ethnic majority group indicator, formal education indicator, a household asset index, and their respective interactions with the white-man variable. Village-level controls include village size, buildings burned during the war, indicators for whether there is a market, a labor gang, and a communal farm, and their interactions with the white-man treatment. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 7. Mechanism: Alternative measures of aid exposure and generosity

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving
White-man	1,533.107	924.094	1,164.019	263.498
	(1,331.817)	(1,400.055)	(1,467.768)	(1,771.239)
NGO aid x white-man	-1,995.590**			
	(897.747)			
NGO aid	617.371			
	(549.319)			
Years aid NGO activity x white-man		-19.501**		
		(8.810)		
Years aid NGO activity		3.472		
		(5.686)		
White aid visitor x white-man			-1,169.218*	
			(676.706)	
White aid visitor			659.246	
			(527.564)	
White non-aid visitor x white-man				752.572
				(696.609)
White non-aid visitor				-365.992
				(529.003)
Chief x white-man	-2,210.229**	-2,410.926**	-2,125.378**	-2,198.963**
	(980.342)	(975.375)	(997.207)	(1,014.808)
Chief	681.912	702.898	717.154	794.448
	(641.672)	(621.866)	(634.507)	(670.185)
Secret society leader x white-man	-3,173.311***	-3,206.969***	-3,024.883***	-3,080.946***
	(909.552)	(900.409)	(893.574)	(875.557)
Secret society leader	1,259.282*	1,185.735*	1,178.587*	1,237.287*
	(703.548)	(702.119)	(698.985)	(711.400)
Individual-level controls	Y	Y	Y	Y
Village-level controls	Y	Y	Y	Y
Observations	630	630	619	619

Notes. Robust standard errors clustered at the village level in parentheses. All specifications include district and ethnicity fixed effects. Chief indicates that the respondent is a chief. Secret society leader indicates that the respondent is from the household of a secret society leader. Individual-level controls include respondent age, an ethnic majority group indicator, a formal education indicator, a household asset index, whether the respondent has met a white person 1 to 10 times, never met a white person, as well as their respective interactions with the white-man variable. Village-level controls include village size, buildings burned during the war, indicators for whether there is a market, a labor gang, and a communal farm, and their interactions with the white-man treatment. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 8. White-man presence and participant beliefs

	(1)	(2)	(3)	(4)
	Logit	Multinomial Logit		
	Aid Test	Aid Test over Give Money	Aid Test over Research	Give Money over Research
White-man	0.439**	0.455**	0.392**	0.862
	(0.156)	(0.161)	(0.155)	(0.159)
Observations	664	664	664	664

Notes. Robust standard errors clustered at the village level in parentheses. Columns 1-4 display odds / relative risk ratios for a unit increase in the independent variable. Column 1 displays the odds ratio from a logit regression on the "Aid Test" indicator variable. Columns 2-4 display the relative risk ratios for each pair of choices from a multinomial logit regression on the categorical variable of participant beliefs over the 'aid test', 'give money' and 'research' choices. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 9. Mechanism: White-man presence, aid exposure and beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Logit		Multinomial Logit					
	Aid Test	Aid Test	Aid Test over Give Money	Aid Test over Give Money	Aid Test over Research	Aid Test over Research	Give Money over Research	Give Money over Research
White-man	0.271***	1.238	0.287***	0.980	0.224***	2.552	0.782	2.603
	(0.099)	(1.946)	(0.106)	(1.513)	(0.089)	(5.002)	(0.173)	(3.419)
Years aid NGO-owned facilities x white-man	1.052**	1.059***	1.052**	1.058***	1.055**	1.063***	1.003	1.005
	(0.022)	(0.021)	(0.022)	(0.021)	(0.024)	(0.024)	(0.005)	(0.006)
Years aid NGO-owned facilities	0.965***	0.969***	0.967***	0.970**	0.960***	0.962***	0.993**	0.992**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.014)	(0.013)	(0.004)	(0.004)
Chief x white-man				1.665		1.345		0.808
				(2.458)		(2.944)		(1.504)
Chief				0.000***		0.000***		2.644
				(0.000)		(0.000)		(2.627)
Secret society leader x white-man		1.961		1.555		5.832		3.751
		(2.342)		(1.862)		(9.360)		(5.116)
Secret society leader		1.363		1.404		1.191		0.848
		(1.172)		(1.177)		(1.340)		(0.589)
Individual-level controls	N	Y	N	Y	N	Y	N	Y
Village-level controls	N	Y	N	Y	N	Y	N	Y
Observations	664	629	664	640	664	640	664	640

Notes. Robust standard errors clustered at the village level in parentheses. Columns 1-8 display odds / relative risk ratios for a unit increase in the independent variable. Columns 1-2 display the odds ratio from a logit regression on the "Aid Test" indicator variable. Columns 3-8 display the relative risk ratios for each pair of choices from a multinomial logit regression on the categorical variable of participant beliefs over the 'aid test', 'give money' and 'research' choices. Individual-level and village-level controls are the same as those listed in Table 7. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Table 10. Mechanism: Alternative measures of aid exposure and beliefs

	(1) Aid Test over Give Money	(2) Aid Test over Research	(3) Give Money over Research	(4) Aid Test over Give Money	(5) Aid Test over Research	(6) Give Money over Research	(7) Aid Test over Give Money	(8) Aid Test over Research	(9) Give Money over Research
White-man	0.811	1.191	1.469	0.957	2.470	2.581	0.248	0.568	2.291
	(1.241)	(2.305)	(1.962)	(1.507)	(4.871)	(3.362)	(0.400)	(1.180)	(2.733)
NGO aid x white-man	7.040***	35.128***	4.990***						
	(4.278)	(26.999)	(2.247)						
NGO aid	0.489*	0.202***	0.414***						
	(0.210)	(0.114)	(0.108)						
Years aid NGO activity x white-man				1.062***	1.067***	1.005			
				(0.020)	(0.023)	(0.006)			
Years aid NGO activity				0.970***	0.964***	0.993*			
				(0.011)	(0.013)	(0.004)			
White aid visitor x white-man							20.295***	13.127***	0.647
							(17.198)	(11.348)	(0.290)
White aid visitor							0.653	1.429	2.189**
							(0.327)	(0.872)	(0.723)
Chief x white-man	2.218	2.040	0.920	1.881	1.621	0.862	1.225	1.239	1.011
	(3.032)	(4.390)	(1.730)	(2.787)	(3.564)	(1.606)	(1.532)	(2.542)	(1.929)
Chief	0.000***	0.000***	2.369	0.000***	0.000***	2.521	0.000***	0.000***	2.105
	(0.000)	(0.000)	(2.367)	(0.000)	(0.000)	(2.497)	(0.000)	(0.000)	(2.148)
Secret society leader x white-man	1.840	7.633	4.149	1.445	5.550	3.841	0.939	4.034	4.296
	(2.076)	(11.265)	(5.627)	(1.717)	(8.907)	(5.241)	(1.220)	(6.749)	(5.887)
Secret society leader	1.134	0.845	0.745	1.381	1.159	0.839	1.240	0.946	0.763
	(0.844)	(0.808)	(0.512)	(1.149)	(1.299)	(0.585)	(0.939)	(0.954)	(0.537)
Individual-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Village-level controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	640	640	640	640	640	640	629	629	629

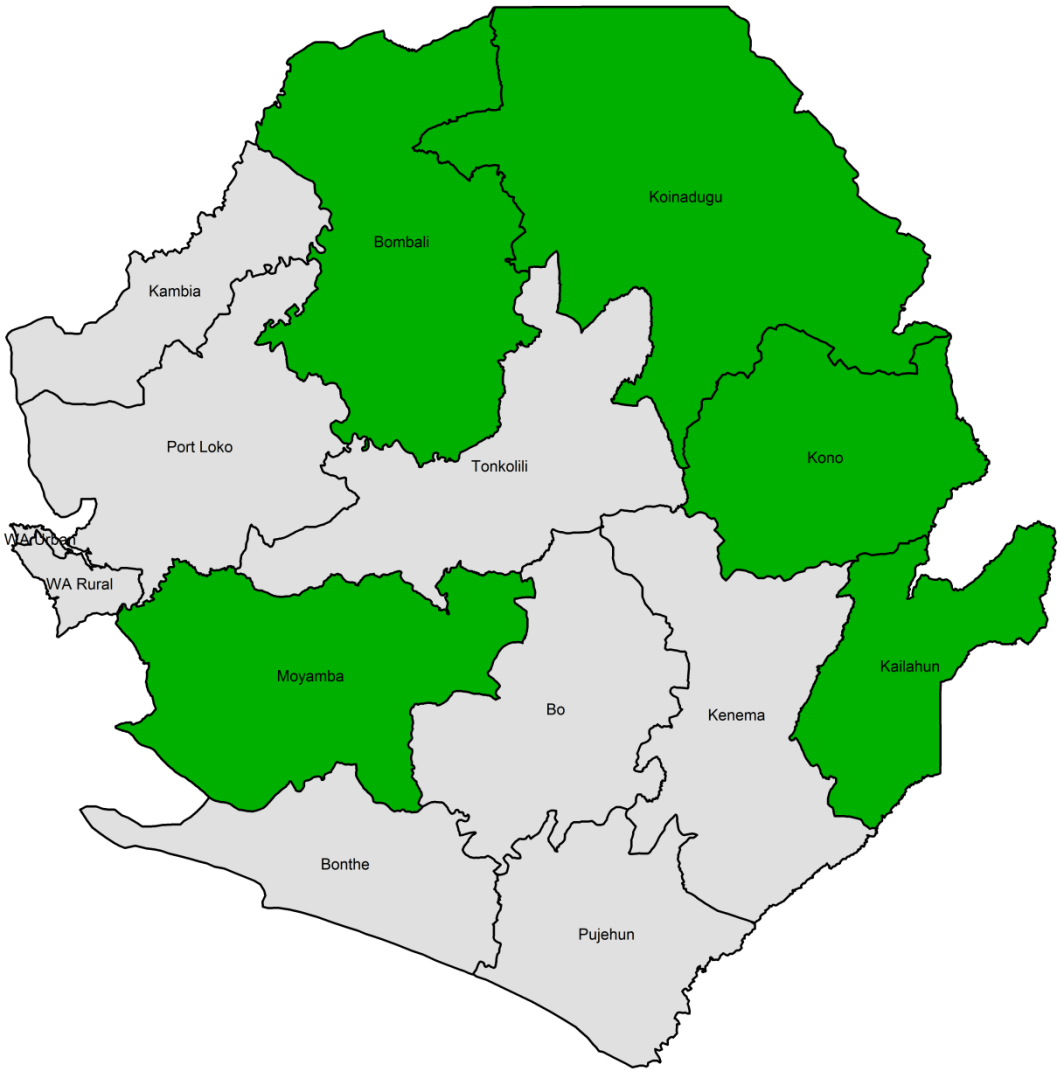
Notes. Robust standard errors clustered at the village level in parentheses. Columns 1-9 display the relative risk ratios for each pair of choices from a multinomial logit regression on the categorical variable of participant beliefs over the 'aid test', 'give money' and 'research' choices. Individual-level and village-level controls are the same as those listed in Table 7. All specifications include district and ethnicity fixed effects. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Appendix Table. Robustness checks

	(1) Total giving	(2) Total giving	(3) Total giving	(4) Total giving	(5) Total giving	(6) Total giving
White-man	604.539**	555.552**	564.943*	542.736*	564.681*	555.390*
	(282.152)	(277.340)	(296.734)	(291.258)	(297.182)	(293.607)
Age	8.200					
	(6.744)					
Village has communal farm		-45.387				
		(424.801)				
Replacement					30.307	-40.539
					(294.568)	(406.934)
Replacement x white-man						127.623
						(630.268)
Ethnicity fixed effects	Y	Y	Y	Y	Y	Y
Linguistic group for ethnicity f.e.s	Primary	Primary	Parent-4	Parent-3	Primary	Primary
Observations	700	708	708	708	708	708

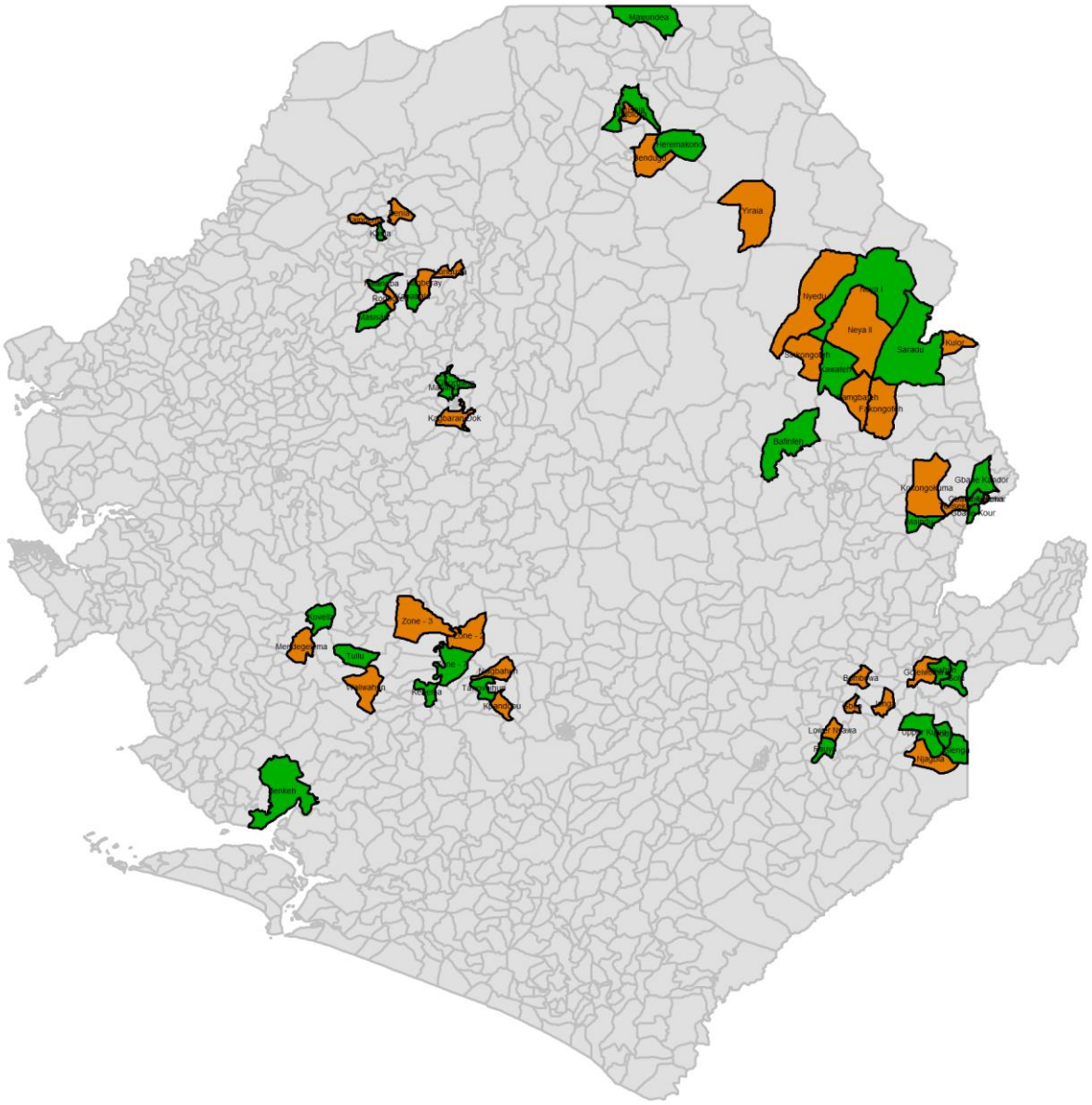
Notes. Robust standard errors clustered at the village level in parentheses. All specifications include district fixed effects. Columns 1, 4 and 5 use ethnicity fixed effects based on the primary local language. Columns 2 and 3 use alternative measures of ethnicity constructed from the 3rd (Parent-3) and 4th (Parent-4) highest parent group of the Atlantic language family. Replacement equals one if the game participant was a replacement household member. *** is significant at the 1% level, ** is significant at the 5% level and * is significant at the 10% level.

Figure 1. Intervention districts



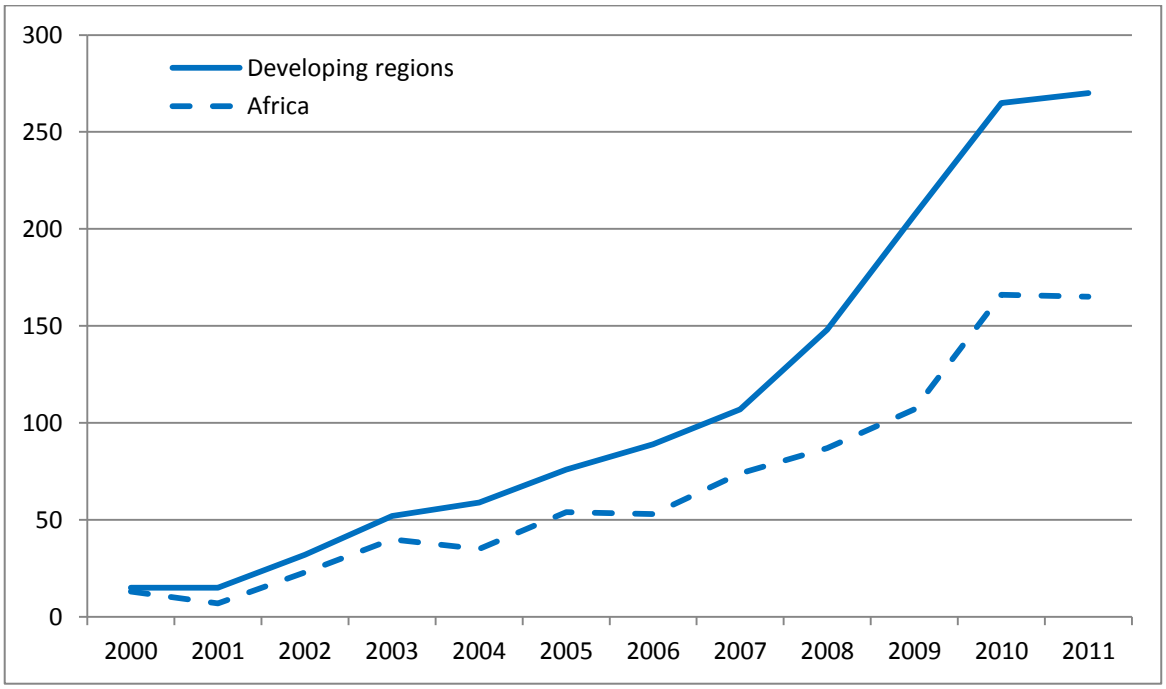
Notes. The five districts of Sierra Leone included in the field experiment are shown in green.

Figure 2. Treatment and control communities



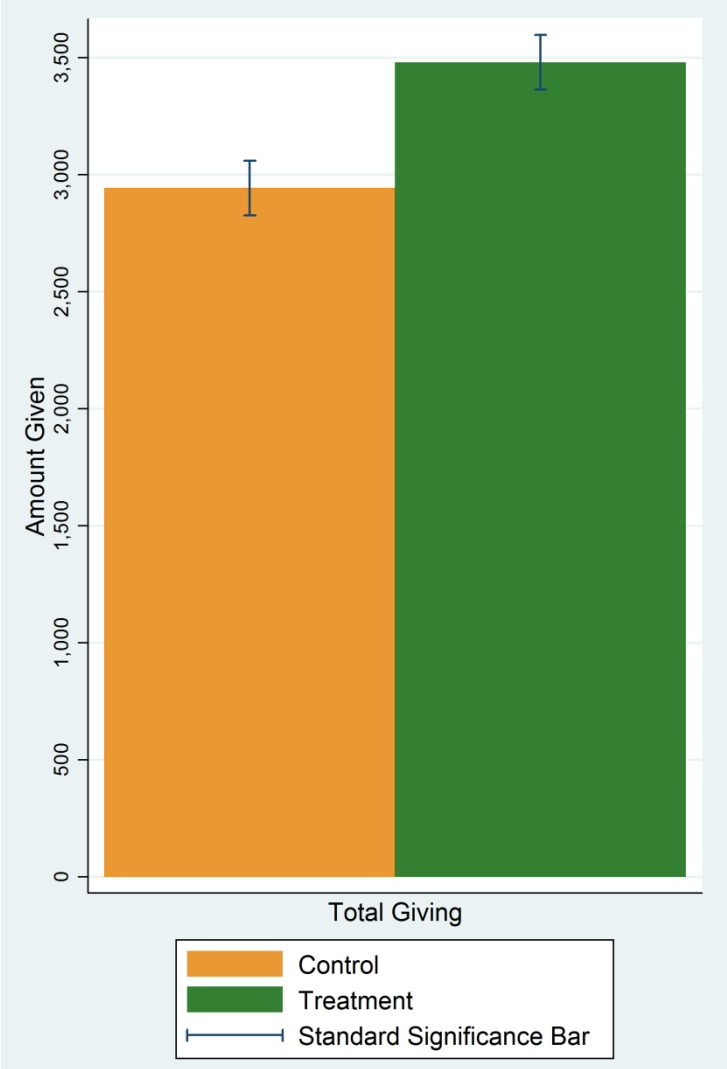
Notes. This figure shows the 30 treatment sections in green and the 30 control sections in orange. One pre-designated village from each of these sections forms the treatment and control communities in the study.

Figure 3: The increasing use of dictator games in developing countries



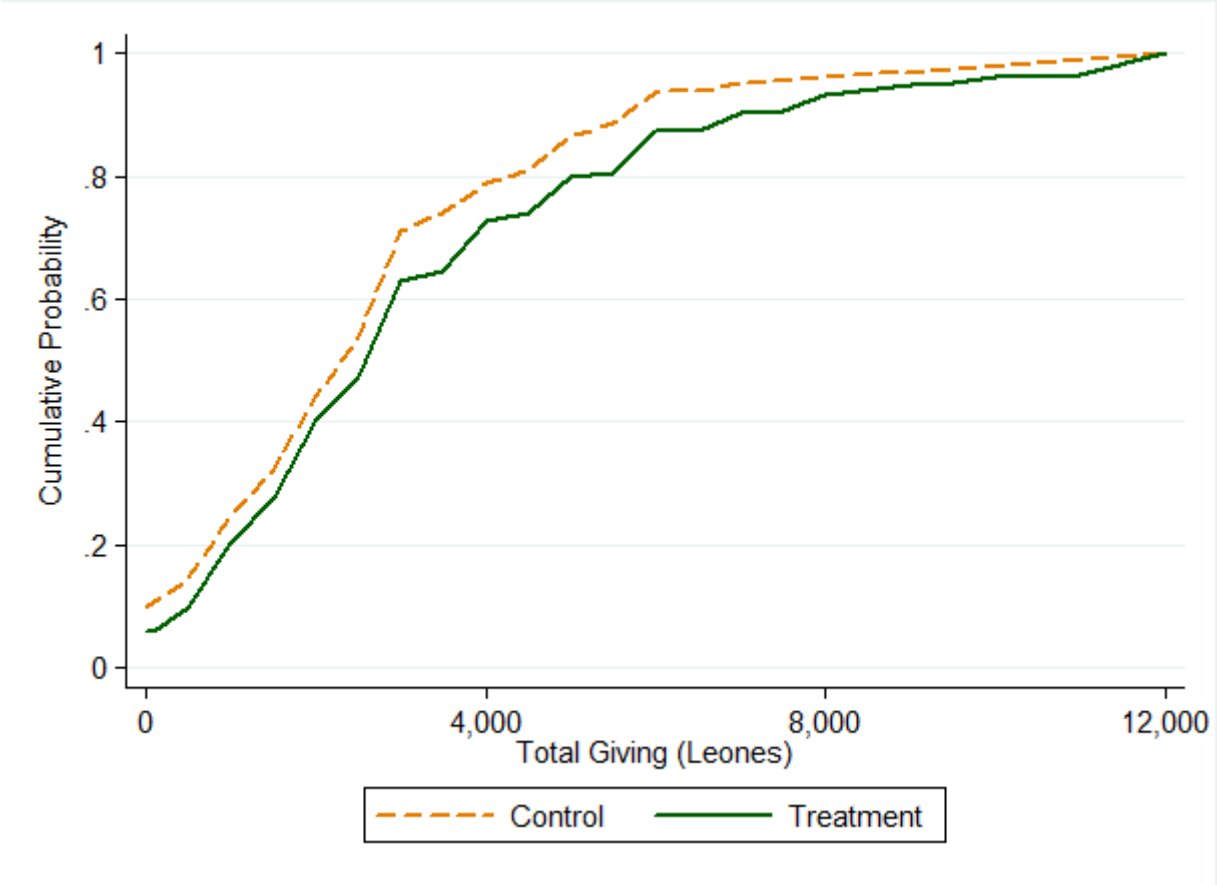
Notes. This graph shows the number of mentions of dictator games in developing countries (solid line) and in Africa (dashed line), based on google scholar counts.

Figure 4. Average total giving in control and treatment groups



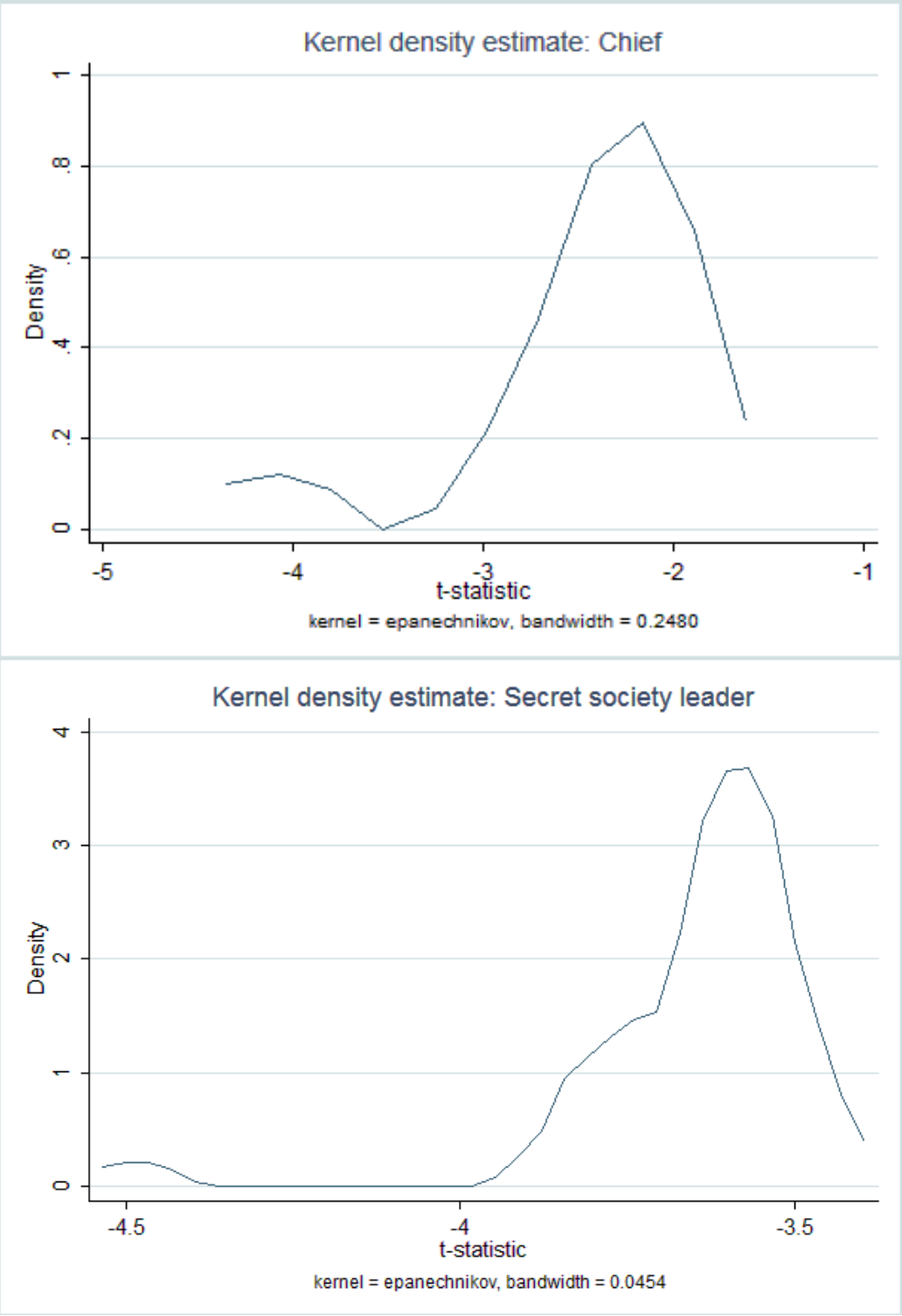
Notes. This figure shows the mean of total giving in the control group and treatment groups. Statistical Significance Bars display pair-wise statistical significance between means using the visual overlap test. Bars are calculated using robust standard errors clustered at the village level.

Figure 5. CDF of total giving in control and treatment groups



Notes. This figure shows the CDF of total giving in the control group and treatment groups.

Figure 6. Robustness check for chief and secret society leader variables



Notes: The figures plot the kernel density estimates of the t-statistics on the interaction term between the white-man treatment and chief and secret society indicators, from regressions dropping each of the respondents who are chiefs (top panel) and related to a secret society leader (bottom panel).